



1985 Hess Medal to Gerald J. Wasserburg

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Citation

This is the first presentation of the Harry Hess Medal. Harry Hess was long Professor of Geology at Princeton. He is most renowned as a founding father of seafloor spreading but contributed significantly to a variety of advances in geology and petrology. Harry Hess died in 1969 after seeing man land on the moon, an event of interest to him as chair of the Space Science Board.

It is an honor that I, a geophysicist, am asked to present the Hess Medal, named for a geologist, to Gerald Wasserburg, a geochemist. I hope I was asked because we share a spirit expressed by Oliver Wendell Holmes, Jr.: "The best service . . . : To see so far as one may and to feel the great forces that are behind every detail . . . to hammer out as compact and solid a piece of work as one can, to try to make it first rate, and to leave it unadvertised." Both Hess and Wasserburg always showed great concern for the significant underlying causes, and both strove to direct not only their own science accordingly but also to urge earth and planetary science in general toward solution of broad important problems.

Hess and Wasserburg differed greatly, not only in primary scientific method—a field geologist and an experimental isotopist—but also in personal manner: one soft-spoken, the other stimulatingly assertive, but they were similar in their concern that others realize their potentials as scientists. My first interactions with both of them reflected this concern. The interaction with Hess was by mail in 1955. I was an army captain in charge of the topographic survey of the island of New

Britain, missing my family and feeling there were better ways to use my talents. So I wrote to a dozen or so geoscience departments, inquiring about opportunities for graduate work. Most of those bothering to reply were in the vein "Forget it. You're better off in the army," but from Princeton, Harry Hess answered with a long letter, discussing what a fine piece of island arc I was tramping on. He didn't have any immediate opportunities to offer but urged me to keep trying. So I did, and so I am here. The first interaction with Wasserburg was in 1962, when I gave a seminar at Caltech [the California Institute of Technology in Pasadena]. I was unusually well prepared (a consequence of taking a bus to get there from Santa Monica), but one member of the audience repeatedly asked questions that helped greatly to clarify the talk. Afterward I asked Bruce Murray who was the interrupter, and got the response "That was Jerry Wasserburg!" "Who's he?" "Oh," (as if to say not to know who was Wasserburg was to be not with it even then).

Not only Jerry's breadth and concern for underlying causes but also his quick interactiveness and his insistence that his colleagues make themselves clear have led to his making important contributions to our science through several committees, most notably the Lunar Sample Analysis Planning Team (LSAPT) and the Committee on Planetary and Lunar Exploration (COMPLEX) of the Space Science Board. LSAPT played the major role in assuring a scientific payoff from the Apollo moon rocks, while COMPLEX laid out NASA's program for exploration of the inner solar system over more than a decade. In particular, he strongly supported exploration of the planets rather than comet missions. I recall his experimental objection: "What can a mass spectrometer get at 30 km/s?" But surely he was influenced by the Holmesian dictum to seek the forces behind the details: these bits of fluff may be similar to planetesimals that went into formation of the outer planets, but they do not afford significant constraint on the origin of the solar system unless and until we get isotopic and inert gas data from them.

Holmes' "leave it unadvertised" may have impressed some of you as inconsistent with Jerry's flamboyance. However, not only has he spent many hours on LSAPT and COMPLEX that do not show on his publication record, but Wasserburg must also be given credit for the foresight that fulfillment of the potential of isotopic techniques required a lot more money and effort. The Apollo project provided the money but only because Wasserburg provided the effort. The results in print included only one paper on the programmable mass spectrometer but many papers on the isotopic character of the moon rocks and Allende and other meteorites and what they meant. To Jerry, the spectrometer is clearly just a means to the end of understanding nature: a means that can consume much time and effort not evident in the final product.

Harry Hess also differed from Jerry Wasserburg in that he came out of World War II as an admiral, while Jerry was a private, a true dogface: Second Infantry Division in the

European campaign. This was an accident of timing: by mid-1944 the military stopped looking at aptitude scores, and youths caught in the meat-grinder thereafter were destined to replace battle casualties, who are mostly in the infantry. I am told that Jerry preserved his status as a private, thereby enhancing his probability of survival, by talking to NCO's and officers with the same directness that a Caltech professor addresses his colleagues, but this is hearsay.

Anyhow, after being mustered out in 1946, Jerry Wasserburg moved rather directly through undergraduate schooling at Rutgers [University] and graduate work at [the University of] Chicago to achieve a faculty appointment at Caltech in 1955. Since then, he has 370 publications on a diversity of topics, a record daunting to summarize. Hence I shall take the escape of describing a half dozen papers, or series of papers, that have particularly influenced me.

The first is a paper in 1964, coauthored by MacDonald, Hoyle, and Fowler, that pointed out that the consistency of the earth's heat flow with chondritic composition of radioactives must be a coincidence, since earth rocks differ from chondrites in their potassium: uranium ratio by a factor of an eighth. So the earth must be relatively enriched in refractories. This "Wasserburg" model has prevailed in evolutionary studies ever since, despite oscillations in opinion as to the relative contributions of radioactive and primordial heat.

The second is a collaboration with Rick O'Connell, around 1970, on models of geosynclines, exploring the interaction of sedimentation rate, isostasy, thermal blanketing, etc. This work demonstrated both Jerry's proficiency as a mathematical modeler and his readiness to go with a good graduate student on a problem removed from his own primary research interests.

The third is the great effort with Papanastassiou, Tera, and many others to establish the lunar chronology by several techniques: Rb/Sr, Pb/Pb, K/Ar, etc. In about 75 papers 1970–1977, not only were the radiogenic ages of many returned samples determined, to give the framework for maria evolution by crater techniques, but also model ages pertaining to earlier times were inferred. Thus from the Pb/Pb and Rb/Sr work it was rather conclusively shown that separation of the lunar crust from the mantle must have occurred at least 4400 million years ago. This completion of formation of a thick crust so soon after origin of the planetary system, together with the dearth of potassium in the moon, demands a very hot beginning for the moon, a belief that has persisted ever since.

The fourth is the long-term attempt, since the late 1950's, to outline the grand chronology of solar system material, in collaboration with Schramm and several others. This attempt necessarily entails models of nucleosynthesis throughout the evolution of the galaxy. Probably the most conclusive achievement is the demonstration in 1969 of a formation interval of about 200 million years between cessation of iodine and plutonium nucleosynthesis and retention of xenon in meteorites.

The fifth is the bonanza of isotopic findings that developed when the Apollo-funded capability was applied to Pueblo de Allende, work done since 1975 with the collaboration of Papanastassiou, Lee, and others. These findings include the establishment of anomalously high magnesium 26:24 ratio in aluminum-rich minerals. The abundances of the extinct radionuclide aluminum 26 indicated thereby entail a formation interval of not more than 3 million years, using prevalent nucleosynthetic models. This material constitutes about 10^{-4} of the solar system. Perservation of these anomalies is a major cosmochemical constraint, indicating a heterogeneous origin for the planetary system: a quick, slam-bang collapse rather than a neat homogenizing accretion disk.

The final area that I would like to mention is the application of Nd:Sm studies, together with Rb:Sr and other ratios, to the evolution of the earth, which has occurred since 1976 in collaboration with DePaolo, Jacobsen, McCulloch, and others. This development required further advances in technique, but characteristically, the emphasis in publication has been on the implications as to evolution of the mantle. In particular, Wasserburg and DePaolo established that the depleted reservoir of ocean rise basalts must have been separated from the richer reservoirs of continental and ocean island basalts for more than 2000 million years. The enriched reservoir is somewhat larger than the depleted. The evident suggestion is that the former is the lower mantle, while the latter is the upper, above the 670-km discontinuity, but there are several arrangements of pipes, valves, and tanks which could satisfy the data, and it will be some time before the fluid dynamicist's models catch up with this challenge.

One of the rock suites examined in these studies of earth evolution is the Stillwater complex: that intricate layering with which geologists and geochemists like to confound geophysicists who think nature is indiscriminating in its ways. A piece of Stillwater forms the base of the Hess medal, which is quite appropriate since Harry Hess also has been one of many to ponder this puzzle.

Some of you may have thought when I spoke of Jerry's urging others to clarify this assertions that he could do more to make his own papers less obscure. Perhaps, but striving for clarity may sometimes sacrifice the more important property of originality. Here I fall back on another guru from outside science, Marshall McLuhan: "Most clear writing is a sign that there is no exploration going on. Clear prose indicates the absence of thought." With Wasserburg, there is always new thought going on, so when he speaks or writes, one can never relax to familiar ideas but must be alert to the new and hence often difficult. Thus upon presenting this first Harry Hess medal, I am waiting, with some trepidation as well as anticipation, to hear what he has to say now.

William M. Kaula

Acceptance

Mr. President, ladies and gentlemen, friends. It is a particular pleasure to share the podium with some old friends, one of whom has just received the senior medal of our society. It is a great honor and privilege for me to receive this Hess Medal. I would like to thank the selection committee, the

council, and the president of the Union for their generous judgment. I can *only* accept this award as a symbol of recognition of the work and ideas shared by my students and colleagues, who have participated in pursuing the elusive history and evolution of the planetary bodies of the solar system.

Geophysics is no longer restricted to seismic wave propagation and the gravity field, with occasional consideration of heat flow and a peripheral reference to iron meteorites. Today, geophysics is the study of the earth and the whole solar system by all of the observational and theoretical tools of a mature and advancing science. The present award symbolizes the integration of all approaches—including isotopic and petrochemical—that provide a fundamental view into the structure and evolution of planetary bodies. The past decades have produced a series of scientific revolutions in which the centripetal force of discovery, analysis, and ideas has driven us together. We are using diverse methods to understand the earth and the planets. We have been through the excitement of the Apollo program and our first sound study of another planet, with new models of a planet, its history and evolution, and its connection with the earth. We have had the privilege of participating in the discoveries of isotopic anomalies and of the short-lived nuclei ^{26}Al and ^{107}Pd and connections between the solar system and the interstellar medium. More recently, there have been the isotopic discoveries and concepts of earth structure and the time scales for forming the depleted part of the mantle.

I take great personal delight in hearing seismologists argue vigorously about the rarest of isotopes in the earth, with geochemists arguing about convective transport across the 650-km discontinuity. The disdain, or possible fear, of "the other" discipline area has been replaced by both a need and a willingness to cross discipline boundaries with ideas and data. This represents the best sense of the American Geophysical Union. I remember the AGU meeting in Washington in 1976 when DePaolo and I, [with] Richard, Shimizu, and Allegre, presented the first Sm-Nd data on terrestrial rocks and the implications for mantle structure and evolution. The session was in a very small room (smoke-filled in that era); people were sitting on the floor and some were just about hanging from the light fixtures. I asked why we could not have gotten a larger room. The response was: We are VG & P [Volcanology, Geology, and Petrology], and we are just lucky to get the space we got. After all, we are not a major part of AGU! It is natural that truly new ideas come out of small rooms of confusion and discovery to more mature forums that fill large halls with intellectual enlightenment and confusion.

The reasons for which I am standing here are probably as unclear to you as they are to me. For the sake of any young people who are present tonight, I feel obliged to enunciate some paradigm that can be used for your guidance. Like Bill Cosby's father, I can tell you how I regularly walked to school 10 miles through a blizzard, uphill, both ways. Some of that is true. Most all of the innovative things I have worked on were rejected by the standard funding agency. The Lunatic I spectrometer was started with Sloan Foundation funds and war surplus parts. NASA [National Aeronautics and Space Administration] sup-

port then followed to build a lab and really get the job done. As far as reviews are concerned, the proposal by Don DePaolo and me to do Sm-Nd on terrestrial rocks (just with funding to support his thesis) was reviewed with a comment that it might be of some interest for a year, but that it was inconceivable that such work would, of itself, be acceptable as a full Ph.D. thesis. Any results would obviously be of doubtful general utility.

I recommend that to preserve our youth, we must keep trying to do the new or innovative things that are not easily accepted. For example, I am once again indebted to this process that keeps me young—a rejection of a proposal to study Nd isotopes and REE [rare earth elements] in sea waters and, most recently, rejection of a paper on the subject by JGR [Journal of Geophysical Research] Oceans. It is just this sort of information a person needs. I then knew that what we were doing was not mundane; it was either stupid or else new and innovative. It is our own critical judgment that tells us what to do then. I have hardly told these stories to elicit your sympathy but rather to say that the road is always bumpy when you try to do something new and that you should hang in there and try to use the bumps for guidance. The real satisfaction that I get is when I am alone and look over some old piece of work and conclude that this is a stone. It is not the mountain, but it is a *very beautiful* stone that I helped polish so that you can see some of its structure. On the rare occasion when I feel that way, I am very satisfied that I helped polish it.

In thinking over the scientific accomplishments in our whole field, I thought about other fields, which are rather specifically focused on problems relating to well-identified equations of motion or transport. Our field is different; it is directed toward nature, which is always full of newness. There have been a stream of advances over the past decades—some observational and some theoretical. We have been moving in big steps. No textbook written 20 years ago will work to guide our students today, and those written 10 years ago are so far out of date in many fundamental considerations as to be of only limited use. This means that our field, the general field of "geophysics," which encompasses the whole solar system and all its components and their origin from and interaction with the galaxy, is a rich and fertile place for exciting fundamental science. It is only limited from our present perspective by a continuing need for inquisitive bright young people and the support for them to function. We have to play on performance and futures. If there are any "young" administrators of science in this audience who wonder what is next, where they should lead us, then I wish to proclaim that the limits of the field are at present bounded by the opportunities to move vigorously ahead. This limit is not from the potential of the field (in spite of Harry Hess's dry wit about a hammer and a microscope being the only tools). We need new inventions of measuring and observing. We need new generations of instrumentation and the support to keep them functioning. We need more groups with an adequate balance in the diverse intellectual, instrumental and technical skills to make the host of discoveries both on the earth and in space. We do not really need more proposals written; we need to do, and have the opportunity to do, more fundamental and exciting science.

Harry Hess and I were old acquaintances and friends. I respected him enormously. We both were denied admission to Princeton. I then went to Chicago, but Harry went to Princeton by mistake. As for Princeton, there were two applicants named Hess, and the wrong one got in. I first met Harry in the early 1950's when I visited Princeton, where he was working with Buddington on the X ray machine studying pyroxenes. When I asked Harry about experimental work at Princeton, he told me that they got all the support they needed by sending students down to the Geophysical Lab [of the Carnegie Institution of Washington] and DTM [Department of Terrestrial Magnetism, also of Carnegie]. Whenever Harry came to California, he would visit with me at Caltech. We would sit outside of the Arms Building on the bench and talk—sometimes of science and sometimes about our philosophy about science. For purposes of argument, Harry would like to claim that all the equipment a geologist needed was a hammer and a microscope. I used to remind the Admiral that he also used the whole U.S. Caribbean fleet, and occasionally a submarine, as logistic support for field work. Other than fencing of this sort, we mostly talked about the seafloor and convection in the earth. He was strongly of the view that the upper part of the earth (in the oceanic section) ran like a tape recorder. The real question was the mechanism. I argued that if you couldn't identify and describe the mechanism, then it was not possible to legitimately interpret things that way. I learned a lot from this. When the plate tectonics revolution later occurred and then developed into a field (and *even* later became an iconography), I learned that the recognition of a general process or form in nature is of itself of great importance, even when the driving and operating mechanism is not fully understood. In complex systems, the use of phenomenological cartoons is often our most powerful guide. We are seeking to understand quantitatively and physically the true mechanisms but must recognize the patterns of phenomena and processes that guide our thought. Ab initio calculations for the evolution of the solar system give great insight, but they usually go off into other universes, not our own. I confess this, even though I love ab initio calculations. In all events the testing of ideas with good critical observations must be the rule. When Papanastassiou and I published our paper on high precision isotopic measurements of Sr in achondrites and early solar system chronology in 1969, Harry told Dick Holland, "this is very important and will lead to major advances." When Dick told me this, I knew that we had received a great compliment.

I thank Bill Kaula for his citation and his quotation from Oliver Holmes with implications about modesty. I have a general rule

about scientists. I have *never* known a modest scientist. Scientists *cannot* be modest. How can a modest person, self-effacing, charge forward to investigate and try to solve a major mystery of nature, a mystery that extends beyond the individual, or the society, in time and space? It seems to me that the activity of science itself is immodest. However, scientists *can* and should be humble. They must be humble in knowing that however hard they try, that they will undoubtedly fail in their own attempt to understand the universe and will only get a somewhat better glimpse of part of the real matrix of truth, if they are very smart and work hard. It is our immodesty that allows us to try to understand and our humbleness that tells us to submit to nature, to its observation and its laws, and to still keep trying. I once heard a speech by a grand unified theorist (GUT) who was reporting on his studies and concluded "that the universe failed to agree with his theory." To me, that represents a lack of both modesty and humbleness.

In closing, I would like to reminisce about some changes from the earlier days of the American Geophysical Union. This is the 24th Western Meeting of AGU. I was one of the organizers of the first Western AGU Meeting in 1961, at UCLA [University of California, Los Angeles]. To announce this, we used as a cover page the October 18, 1850, Friday, 2 o'clock issue of the San Francisco Alta, which announced the admission of California into the Union and also had peripheral reports about the use of inferior gold nuggets at the Monte tables, the arrival of mess pork, Havana cigars, and French wallpaper. The city hasn't really changed. Having a west coast meeting was a major break with tradition, as all meetings had previously been at Washington, D.C., the center of the formal scientific universe in this nation. The balanced growth of activities now has us regularly sharing meetings and now formal functions on both coasts. As a geologist and geophysicist, I am honored and delighted to have participated under these most special circumstances.

Thank you.

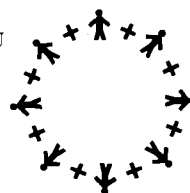
Gerald Wasserburg

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